it. When a cart with a butcher's man came into the place where the dogs were kept, although they could not see him, they all were ready to break their chains. A master-butcher, dressed privately, called one evening on Paris's master to see the dog. He had hardly entered the house before the dog (though shut in) was so much excited that he had to be put into a shed, and the butcher was forced to leave without seeing the dog. The same dog at Hastings made a spring at a gentleman who came into the hotel. The owner caught the dog and apologised, and said he never knew him to do so before, except when a butcher came to his house. The gentleman at once said that was his business. So you see that they inherit these antipathies, and show a great deal of breed.' "WILLIAM HUGGINS"

The unreasonable

MY attention has directed itself to a letter by Dr. Ingleby in your last number, containing two curious but inconsistent misrepresentations of my words, and therein something that, if the writer were not Dr. Ingleby, might be called an instructive instance of cynophatnism or doggimangerness—the behaviour of one who will neither understand a thing himself, nor allow other folk to understand it. As, however, the writer is Dr. Ingleby, I feel sure that a less cursory contemplation of the matter will modify his views.

The following doctrines are in the *Kritik* :—

1. At the basis of the natural order is a transcendental object.

> "Das *transcendentale Object*, welches den aüsseren Erscheinungen, ingleichen das, was der inneren Anschauung zum Grunde liegt, ist weder Materie, noch ein denkendes Wesen an sich selbst, sondern ein uns unbekannter Grund der Erscheinungen, die den empirischen Begriff von der ersten sowohl als zweiten art an die Hand geben." (IVth Paralogism, of Ideality ; *First Edition.*)

2. The transcendental object is *unreasonable*, or evades the processes of human thought.

(*a*) Of the sensibility :—

> "Die nichtsinnliche . . . Ursache dieser Vorstellungen ist uns gänzlich unbekannt, und diese können wir daher nicht als Object anschauen." . . . (VIth section of Antithetic.)

(*b*) Of the understanding :—

> "Unser Verstand . . Dinge an sich selbst (nicht als Erscheinungen betrachtet) *Noumena* nennt. Aber er setzt sich auch sofort selbst Grenzen, sie durch keine Kategorien zu erkennen, mithin sie nur unter dem Namen eines unbekannten Etwas zu denken." (Ground of distinction between Phenomena and Noumena.)

3. The doctrine of the contradictions is one means by which we know this.

> "Mann kann aber auch umgekehrt aus dieser Antinomie . . . die transcendentale Idealität der Erscheinungen . . . indirect . . . beweisen," &c. (VIIth section of Antithetic.)

The Kantian theory had two legs to stand upon ; one the alleged necessity of mathematical axioms, the other these alleged necessary contradictions in our ideas of the natural order. How completely the first has been amputated I hope to have shortly an opportunity of showing in a course of lectures at the Royal Institution. The doctrine, that we may infer the existence of an unknowable from supposed contradictions in the knowable, "has been developed and extended by the great successors of Kant ;" and when in "a later form" these contradictions were set forth from an ultimately empirical standpoint (*not* that of Hamilton, but of Spencer, as stated in my note) the doctrine became fit for notice in a scientific lecture. Only the contradictions themselves, however, could be criticised, and not the step from them to the existence of the unknowable, or the unknowability of the existent. And Kant's name could only be mentioned as the historical starting-point of the doctrine ; whose importance for the empiricist is mainly due to the modifications it has undergone since his time.

If Dr. Ingleby will kindly look at my lecture (*Macmillan's Magazine*, October 1872) again, he will see that I have attributed to Kant no more than the above-quoted doctrines ; that I never pretended to expound Kant's form of them, or their relation to the rest of his system ; and that I never said nor accused anybody of saying either that the antithetic was unreasonable, or that any natural order of thought or things was unreasonable. In regard to the other misrepresentations he speaks of, I shall be very glad indeed to be told of them, and to be set right, provided only they exist in my words, and not in the exuberant imagination of my critic.

London, Feb. 9 W. K. CLIFFORD

P.S.—There is an important error in p. 508 of the lecture in question. The surface-tension of camphor and water is *less* than that of water, not *greater*, as there stated. The general argument depends only on there being a difference.

Prof. Clifford on Curved Space

THE friend, who (as I stated in my letter in NATURE, Feb. 6) called my attention to Prof. Clifford's address in *Macmillan's Magazine* for October last, asked me certain questions respecting curved space, which I was quite unable to answer : and another friend, occupying the foremost place among English philosophers, has since communicated to me the great discomfort which Prof. Clifford's views had occasioned him, and suggested that I should comment upon them in NATURE. I am not sure that what I have to say will prove to be helpful either to my discomforted friend, or to truth : yet the doctrine of curved space is so extraordinary in itself, and so momentous in its consequences, if it be true, that it is a fair subject for sceptical scrutiny. Moreover, I do not conceive that in commenting upon it I am going *ultra crepidam* ; for the nature of space is not a subject on which the mathematician can claim a monopoly. *In limine* allow me to express my regret that Prof. Clifford should have selected such a topic for the entertainment of a popular audience. It is quite incredible that any of his hearers could have apprehended his meaning. There was assuredly no need for the lecturer to have cast a glamour on their mental eye by the invocation of those awful names, Lobatchewsky and Gauss, Riemann and Helmholtz.

The principle, in exemplification of which Prof. Clifford expounded the doctrine in question, was this : that a law can be only provisionally universal (*i.e.* as "we find that it pays us to assume it"), but that it is theoretically universal, or true of all cases whatever, "is what we do not know of any law at all" p. 504. I fancy he would not include numerical formulæ under the term "law :" else arithmetic and algebra would afford an infinity of examples of such a law. Be that as it may, he does not select an example from either of those sciences, but from Euclidian geometry. He takes the proposition established by Euclid, that in any plane triangle the three angles added together are equal to two right angles. This he asserts we do not know as a universal truth. I now quote his own words : "Now suppose that three points are taken in space, distant from one another as far as the sun is from α Centauri, and that the shortest distances between these points are drawn so as to form a triangle : and suppose the angles of this triangle to be very accurately measured and added together ; this can at present be done so accurately that the error shall certainly be less than one minute. . . . Then I do not know that this sum [? apart from the question of error] would differ at all from two right angles ; but also I do not know that the difference would be less than 10°." If, then, after a sufficient number of observations it were found that the deviation were greater than the assigned limit of error (less than one minute), it would follow that the Euclidian law is not universal, and that for triangles of such dimensions it is not true. The conclusion would be, then, that our Tridimensional space is not a homaloid. We need not run our heads against the ghost of a fourth dimension ; for the refinements of the geometer enable him to investigate a curved tridimensional space, just as he investigates a homaloidal tridimensional space. But all the same, it is absurd to attempt the interpretation of the results without supposing that fourth dimension as the *conditio sine quâ non.*

Now we will suppose that the triangle in question has been surveyed, and that the sum of its three angles have been found to deviate from π far beyond the assigned limit of error : what have we really got thereby ? The triangle, says Prof. Clifford, is formed by drawing "lines of shortest distance" between the three points in space. Is observation through a telescope drawing such a line ? Be it so, for the sake of argument. Then, if the conclusion to be drawn is that space is curved, I ask does it or does it not follow that the sides of the triangle are themselves curved ? Observe that if those seeming (to us) straight lines are really curves of an exceedingly small curvature, the Euclidian law is not touched. Of course, then, Prof. Clifford did not mean to assert that in a case in which the sides of a triangle are

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not absolutely straight lines, it does not necessarily follow that the sum of its angles is equal to π: for Euclid himself is quite ready to admit that. No: Prof. Clifford must have meant that those three sides, though *absolutely straight* to us, creatures who can only imagine a homaloidal tridimensional space, are curved in a sense (thanks to a fourth dimension) which vitiates the Euclidian law.

Of course he may disclaim this interpretation: or he may assert that in the case supposed the three sides are both straight and curved, or neither straight nor curved, if such be his view. But until I see his disclaimer I shall hold that he meant to suggest to his audience that straight lines (proved to be so by the standard of straightness which is alone imaginable by creatures constituted as we are) are in another sense really curved, and as such afford an observable exception to the Euclidian Law. Now I say that, constituted as we are, we could have seen straight lines only as straight, and therefore we simply *could not* see those sides otherwise than as verifying that Law; and so we could never bring to the test of observation the question raised by the great quaternion of geometers; and therefore must for ever remain in absolute ignorance whether the space, in which we "live and move and have our being," be (in another relation) something different from what we find it to be in relation to our faculties.

C. M. INGLEBY

Athenæum Club, Feb. 8

Earthquake in Pembrokeshire

I HAVE received a letter from the west part of Pembrokeshire, dated February 3, from which the following is an extract:—

"Last Saturday, at 7 A.M., my bed shook twice under me; and at the same time the servant went into the dining-room, the fire-irons rattled and the room shook; an hour later, ——'s bed shook twice."

I do not know whether any notice has been taken of the occurrence elsewhere. I have paid some attention of late years to the indications of earthquakes in this neighbourhood, and am inclined to think that slight tremulous movements take place more frequently than may have been supposed or recorded. They would naturally be unnoticed in the daytime, and their detection would depend upon accidental wakefulness at night.

Hardwick Vicarage, Feb. 8 T. W. WEBB

Meteorology of the Future

IT is with some satisfaction that I have read in NATURE of December 12, 1872, the very interesting paper of Mr. J. Norman Lockyer, entitled "Meteorology of the Future," giving adhesion and the support of his name to the discovery of Mr. C. Meldrum, of a cycle of 11 years in the recurrence of the maximum of cyclones and rainfall in the southern hemisphere; a cycle corresponding with that already recognised in the maximum of sun-spots. But I have been somewhat surprised to see that my name has not been mentioned by Mr. Lockyer in reference to Mr. Meldrum's paper, as I have also published a paper on the connection of sun-spots with rainfalls, storms, cyclones, &c., prior to the first paper of Mr. Meldrum, which appeared in NATURE, October 24, 1872. Thinking that my paper has escaped your notice, and trusting that you might have some interest to see it, I take the liberty to forward it to you with this same mail. It was published in the *Boston Daily Advertiser*, November 2, 1871. Over a year has elapsed since its publication, and few are the days on which I had no opportunity of seeing the sun and scrutinising its spots with especial care, with the aid of telescope and spectroscope; and to-day I do not see the necessity of changing a word of the conclusions which I had come to in that paper. Only it appears that, in addition to the laws which I have drawn out, the position of the moon will have to be taken in consideration as a complicating element; as it seems that the conjunction and opposition have a tendency to increase the influence of the spots on our atmosphere, while the quadrature diminishes it in a certain measure. I could make some other remarks taken from my greater experience on the subject, but they are of secondary importance, and I will wait for another opportunity to publish them.

Perhaps I did not guard myself sufficiently in my paper, and have not explained with a sufficient amount of clearness, that though the effect of sun-spots on the weather is general all over the globe, yet the result cannot be expected to be absolutely the same; as local causes, very numerous, like mountain chains, forests, rivers, coasts, oceans, and climates, have an independent influence on the distribution of rains and the direction of winds, &c. But local causes are of a secondary order, and will be easily determined when once we are sure that the primary cause of atmospheric disturbances is to be found in the solar spots

L. TROUVELOT

Cambridge, Mass., Jan. 27

Deep Wells

SINCE the question of the supply of water to deep wells was touched upon in NATURE (vol. vii. p. 177), in connection with the rainfall of 1872, I have been in hopes each week of seeing the subject thoroughly and scientifically discussed. It will be recollected that while we were all sneezing and spluttering, and thoughtlessly complaining of the long-continued wet, Mr. Bailey Denton deprecated the premature interference of the Archbishop of Canterbury with the rain, on the ground that the deep wells were not yet filled. This raised a great deal of discussion; people lost their tempers over the rain; and the country seemed to be divided into three bitterly hostile parties—the supporters, the opponents, and the suppliants of Providence. But still the geologists held aloof, and no one even answered the question, "What is a deep well?" but continued to talk as if wells were divided into two classes, deep and shallow, by a hard and fast line.

I therefore venture to hope that some geologist will take up the question in your columns, and give us a few facts instead of opinions. Meanwhile, I will state the case as it appears to me. With the exception of chalk and limestone formations, deep wells are, I believe, unknown in hills. In the side of a hill water comes naturally to the surface in a spring. Wells are only required—or, at all events, deep ones—at a distance from hills. They derive their water from water-bearing strata supplied in all cases either directly from hills, or indirectly from hills through the leakage of river-beds. No amount of rain falls upon cultivated, and therefore comparatively low-lying, land in Europe, sufficient to penetrate to even a shallow well through the earth immediately around it. This, at least, I presume to be the case, for 33 per cent. of their own bulk may be taken as an average amount of water for average soils to be able to retain and hold, so that if a well were 15 ft. deep to the top of the water it could not be affected by less than 5 ft. of rainfall, and when we deduct the enormous proportion of the 5 ft. that would be lost by evaporation and intercepted by vegetation, it is manifest that even 5 ft. of rain could not penetrate 15 ft. through any ordinary average soil. How, then, could any rainfall penetrate to a "deep" well of, say 100 or 200 ft. in depth?

Feb. 9 W. HOPE

THE GRESHAM LECTURES ON PHYSIC

THE Hilary Term Course of Lectures on Physic were delivered at the Gresham College, Basinghall Street, by Dr Symes Thompson, on the evenings of the 17th and 18th ult., and the subject of the discourses upon this occasion was the important and interesting one of Contagious and Infectious Diseases. The professor started on his career of familiar explanation by describing two recent instances of outbreak of infectious disease in rural districts, in which the introduction and march of the fell agent of communication through the ranks of the small community could be distinctly traced. In the one case, the infection of scarlet fever was brought to the village of Flindon, in Hampshire, by a girl who came from Worthing, and served in a small general shop which was resorted to by all the villagers. Only two houses in the village that had children in them, escaped from the disease. In the other case, enteric fever was taken to Whitchurch, in Hampshire, by a young woman from Basingstoke, who returned to Basingstoke to die, after only six days' sojourn in Whitchurch. The fever, nevertheless, spread from the house in which she stayed, and within the next seven months there were seventy cases of enteric fever in a small community numbering only 1,450 people. The instance at Whitchurch acquired especial importance and interest, because it was made the ground for an investigation and report by the Local Government

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